



STANFORD UNIVERSITY MEDICAL CENTER

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STANFORD UNIVERSITY SCHOOL OF MEDICINE  
Department of Genetics

Dr. Arnold W. Ravin  
Department of Biology  
The University of Chicago  
1103 East 57th Street  
Chicago, Illinois 60637

Dear Arnold,

Thank you for your very interesting and useful material vis-a-vis Harriett. I think the documents that you sent do pin down several of my questions quite clearly. I would guess that if Harriett gave a seminar in late 1942 that it was in the context of the discussion in Doby's book rather than other direct contact with Avery, although it would be interesting to pursue this. Quite possibly she was already very much sensitized to the problems of pneumococcus transformation even before that 1944 publication.

My review in the American Scientist, reference 13, summarizes the possible diverse interpretations of the phenomenon. It is interesting that in his letter to his brother Avery presents a much clearer picture of the genetic significance of transformation than anything Doby wrote in his book. I have to agree that Dunn was her supervisor; she also acknowledges him and Hecht in her publication on yeast. I will have to write to Dunn to see if he can recall how in the world he came to agree to sponsor a thesis on growth in yeast, which seems a rather improbable topic from almost any criterion that one would use today.

I would be very much interested to know what you meant by your plans for a further exploration of the Griffith-Avery story. I have in mind to write a somewhat more detailed note than has appeared so far about the background of the investigation of bacterial recombination, and I would be delighted to be able to refer to a reliable account of the Avery stuff, particularly one that I agreed with. My letter to Nature in reply to Wyatt is probably going to seem a little bit too defensive about how geneticists took that story, and of course I guess one has to specify which geneticists! What I really do back at is the idea that he seems to be promoting that Avery's work was simply not known about, which is, of course, preposterous. The idea that bacteria were suitable objects for genetic investigation is another story, and I guess we all had our knocks about that.

I have no doubt at all that it was Dunn and Dobzhansky who pooh-poohed Harriett's interest in pneumococcus. It was common knowledge that Francis Ryan had his knocks on rather similar questions; and ~~what~~ I know better now than then was how precarious Francis must have thought his career might be in the face of these sorts of obtuseness about microorganisms and about biochemical approaches to genetics that pervaded some of those seniors.

¶ My own debt to her is quite clear. I would be interested in anything else that you can remember or dig up that might reflect on the influence that Harriett and Francis had upon one another. Can you remember anything of what he might have thought about Avery independently of Harriett, or would his insight into

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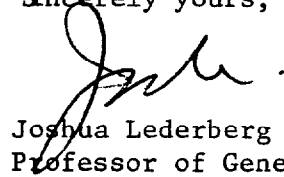
Dr. Arnold W. Ravin

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that have been pretty much derivative of hers?

Sincerely yours,

A handwritten signature in dark ink, appearing to read 'Joshua Lederberg', written over the typed name.

Joshua Lederberg  
Professor of Genetics

JL/rr